Exhibit 25

CHAPTER 17A

EVENT STUDY METHODS: DETECTING AND MEASURING THE SECURITY PRICE EFFECTS OF DISCLOSURES AND INTERVENTIONS (NEW)

M. Laurentius Marais, PhD
Katherine Schipper, PhD

CONTENTS

17A.1 INTRODUCTION. A dispute or litigation involving the value of a company often requires analysis of price changes of public securities in response to news about, for example, product recalls, takeover offers, regulatory and legislative changes, and earnings. In particular, substantial litigation arises in the area of

securities fraud: Typically, a class of investors sues a company and some or all of its officers, directors, and professional advisors for losses suffered when the security price declines, usually after the release of adverse information.

When a dispute or litigation requires an analysis of security price changes in response to a news disclosure, it is often appropriate to distinguish two related questions: (1) Does the price history establish that the disclosure caused any price reaction at all?, and (2) if so, what was the size and direction of the net price reaction to the disclosure alone, after accounting for the effects of other factors operating at the same time? This chapter describes a statistical approach, often referred to as an *event study*, for analyzing these questions. We outline the creation and interpretation of a generic, market-model-based event study without specifying a particular context or purpose. Chapters 18 and 19 of the third edition of the *Litigation Services Handbook* describe potential litigation contexts for such calculations, such as the quantification of damages by combining event study results with information about trading.

Computing damages in securities cases is an important, litigation-related application of event studies. The literature reviewing the legal and economic theories supporting this application include Gilson and Black (1993), Macey and Miller (1990), Koslow (1991), Cooper Alexander (1994), and Macey et al. (1991). Other articles provide additional details concerning event studies and the translation of an event study result to a dollar damage figure: Cone and Laurence (1994), Furbush and Smith (1994), and Gould and Kleidon (1994).

(a) Definition of Event Study. An *event study* is an empirical analysis of an intervention in a time series. In its most common form, an event study involves a statistical regression analysis of a time series of security returns, with the objective of identifying and measuring firm-specific effects of identifiable information releases (events). (Unless otherwise noted, we will use the terms *stock, share,* and *security* interchangeably). While many event studies analyze common stock or securities, the approach also applies in other contexts, some of which this chapter describes.

(b) Illustrative Example. Exhibit 17A-1 lists the daily closing prices and returns of the shares of the hypothetical Firms A and B for a 51-day period surrounding a hypothetical announcement date. (The data are artificial but illustrate typical patterns.) Exhibit 17A-2 plots the time series of share prices for Firm A shown in Exhibit 17A-1. This display strongly suggests that some event positively affected the price of Firm A's shares at day 0 (zero), the day of the announcement. Day 0 is referred to as the *event day*. Such impressive interventions usually occur only in conjunction with extraordinary events such as receipt of a takeover offer. Large *negative* price adjustments may also occur, for example, in association with unexpected adverse earnings news. [See, for example, Francis et al. (1994) and Kasznik and Lev (1995).]

Simple displays of real-world data do not usually reveal such obvious features. To confirm or refute the hypothesis of an extraordinary intervention in a series, an analyst will usually have to do more than plot and inspect the data. Often, he or she will use formal statistical analysis both to decide whether an intervention has occurred and to measure the size of its effect.

Event Date	Firm A Closing Price	Firm A Stock Return	Firm B Closing Price	Firm B Stock Return	Market Return	Event Date	Firm A Closing Price	Firm A Stock Return	Firm B Closing Price	Firm B Stock Return	Market Return
						0	$56.81	12.01%	$39.97	-1.24%	1.40%
-25	$50.00	0.01%	$34.00	-2.86%	-1.41%	1	$57.03	0.39%	$40.25	0.70%	1.41%
-24	$48.03	-3.94%	$34.09	0.26%	-0.57%	2	$55.28	-3.07%	$39.72	-1.32%	-0.85%
-23	$45.22	-5.85%	$35.16	3.14%	1.39%	3	$54.88	-0.72%	$39.47	-0.63%	-0.30%
-22	$47.31	4.62%	$34.69	-1.34%	-0.49%	4	$54.47	-0.75%	$37.97	-3.80%	-2.00%
-21	$46.94	-0.78%	$34.59	-0.29%	-0.55%	5	$53.16	-2.40%	$37.44	-1.40%	-0.75%
-20	$48.34	2.98%	$34.25	-0.98%	-0.78%	6	$53.88	1.35%	$37.03	-1.10%	-0.06%
-19	$47.72	-1.28%	$34.25	0.00%	-0.09%	7	$55.03	2.13%	$37.66	1.70%	1.49%
-18	$47.34	-0.80%	$34.75	1.46%	-0.48%	8	$57.31	4.14%	$37.19	-1.25%	-0.45%
-17	$47.78	0.93%	$34.69	-0.17%	-0.50%	9	$56.25	-1.85%	$36.91	-0.75%	-0.92%
-16	$47.16	-1.30%	$34.81	0.35%	0.67%	10	$57.22	1.72%	$37.22	0.84%	0.63%
-15	$48.16	2.12%	$34.69	-0.34%	-0.35%	11	$57.59	0.65%	$37.16	-0.16%	-0.44%
-14	$48.66	1.04%	$35.56	2.51%	1.23%	12	$58.94	2.34%	$37.84	1.83%	1.60%
-13	$48.34	-0.66%	$36.69	3.18%	1.17%	13	$58.97	0.05%	$37.50	-0.90%	-0.66%
-12	$47.38	-1.99%	$38.25	4.25%	0.07%	14	$59.97	1.70%	$37.53	0.08%	-0.81%
-11	$48.44	2.24%	$37.69	-1.46%	-1.33%	15	$60.09	0.20%	$37.56	0.08%	-0.34%
-10	$47.72	-1.49%	$37.97	0.74%	0.57%	16	$60.41	0.53%	$36.94	-1.65%	-0.79%
-9	$46.91	-1.70%	$38.00	0.08%	0.96%	17	$61.16	1.24%	$36.91	-0.08%	1.62%
-8	$46.84	-0.15%	$38.28	0.74%	0.79%	18	$61.53	0.60%	$38.38	3.98%	1.33%
-7	$48.44	3.42%	$37.63	-1.70%	0.19%	19	$60.69	-1.37%	$37.81	-1.49%	-0.31%
-6	$49.31	1.80%	$37.50	-0.35%	-0.10%	20	$61.00	0.51%	$37.84	0.08%	0.38%
-5	$49.47	0.32%	$38.28	2.08%	0.28%	21	$60.09	-1.49%	$40.19	6.21%	1.16%
-4	$48.50	-1.96%	$38.53	0.65%	0.18%	22	$61.97	3.13%	$39.94	-0.62%	1.18%
-3	$48.34	-0.33%	$38.63	0.26%	-0.72%	23	$62.59	1.00%	$40.13	0.48%	0.13%
-2	$50.03	3.50%	$39.75	2.90%	1.42%	24	$62.41	-0.29%	$40.38	0.62%	0.16%
-1	$50.72	1.38%	$40.47	1.81%	1.18%	25	$60.16	-3.61%	$40.69	0.77%	-0.64%

Exhibit 17A-1. Daily Closing Stock Prices and Returns for Periods of 51 Trading Days Surrounding Announcement Events Firms A and B

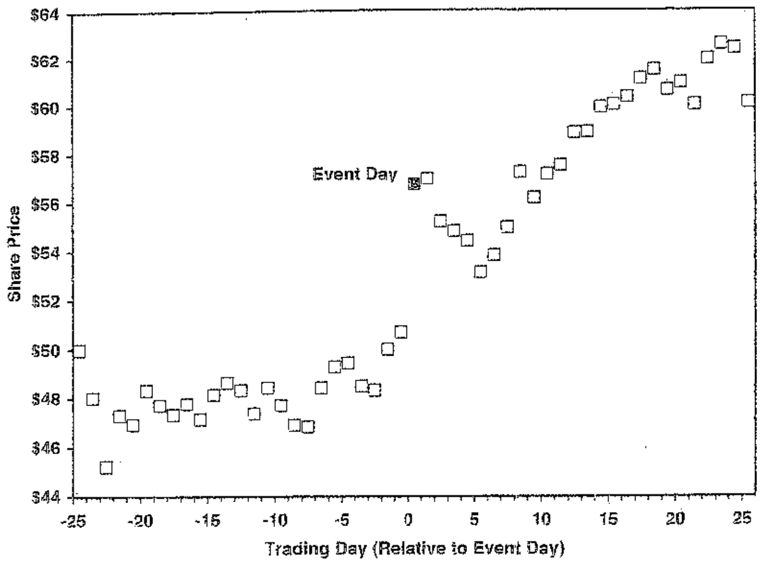

Exhibit 17A-2. Share Price of Firm A: Announcement at Day 0

Even absent specific, identifiable interventions, security price series display variation, as prices move in response to news about the economy in general, specific industries, and individual firms. Statistical methods provide objective interpretations of the evidence for—and estimates of the size of—the effect of a given intervention or shift in a price series, against its background variability. These statistical methods typically provide interpretations of the data that rely on an underlying statistical model which assumes *stationarity:* that each observed data point is drawn (independently) from a probability distribution of feasible values that does not change from one observation to the next. This clearly does not hold true of the prices in Exhibit 17A-2, in that the price on any given day tends to be closer to the prices at nearby dates than to those at remote dates. This indicates that the probability distribution of prices changes over time, in other words, it is *nonstationary.*

When referring to the stationarity assumption, one must distinguish between prices and price changes. Research on the behavior of security prices has shown that successive relative price *changes,* or *security returns,* are approximately independently drawn from a stationary distribution.[1] Thus the distribution of *returns* conforms well to the assumption underlying the statistical methods used in event studies, even though the distribution of *prices* does not. Exhibit 17A-3 shows the series of raw returns corresponding to the price series in Exhibit 17A-2. The return for any given nonevent day in Exhibit 17A-3 is no more likely to be similar to that for a nearby date than to that for a remote date. Thus, the structure of the return

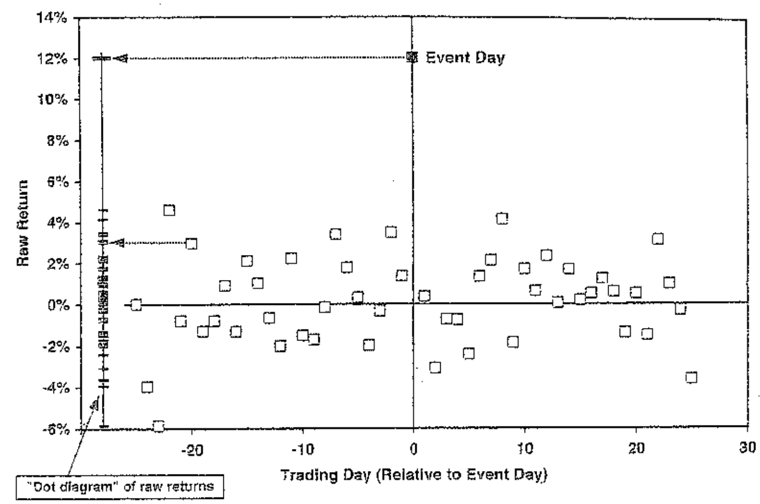

Exhibit 17A-3. Raw Returns to Firm A: Announcement at Day 0 (Dot Diagram of Raw Returns Obtained by Sliding All Data to Vertical Axis)

series is consistent with random sampling from a stationary distribution: the ordering of the nonevent returns in a time series does not relate to their values in Exhibit 17A-3.

The statistical analysis of the Firm A security return on the event day presents a formal method for comparing that return to the range of nonevent returns and their relative frequencies, which provide a benchmark for the range of normal return variation. Exhibit 17A-3 shows a vertical dot diagram obtained by projecting all the return observations onto the left-hand vertical axis, as indicated by horizontal arrows for an illustrative observation at day −20 and for the event date. The dot diagram visually displays frequency by density. An informal comparison of the event-day return to the nonevent dot diagram identifies the event-day return as highly unusual. The analyst can formalize this indication as a special case of a pooled, two-sample t-test. The intervention on the event day stands out as impressively in the returns series shown in Exhibit 17A-3 as in the price series shown in Exhibit 17A-2, so that formal statistical analysis will likely confirm what the data display appears to reveal on its face.

Often, analysts cannot easily discern the event-day return, as Exhibit 17A-4 illustrates. The event-day return to Firm B (shown in both the time series plot and the dot diagram in solid black) does not present the most extreme observation in Exhibit 17A-4.

Inspection alone does not reveal whether the event day return to Firm B is so unusual as to offer evidence of an intervention causing a material market reaction

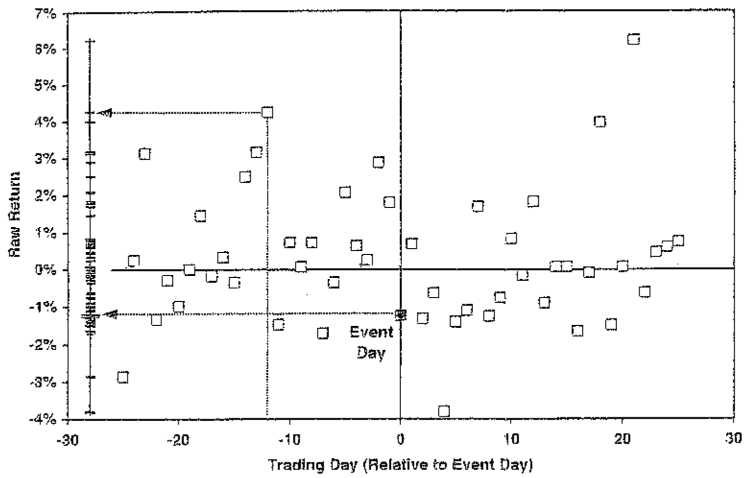

Exhibit 17A-4. Raw Returns to Firm B: Announcement at Day 0

to the announcement. Interpretation of the data underlying Exhibit 17A-4 must therefore rely on statistical inference. Again, assuming that the returns are independently drawn from a stationary distribution, their ordering has no relevance. Thus, the problem of assessing the evidence for a material market effect reduces to that of comparing the event-day return to the nonevent dot diagram of returns obtained by projecting all the data onto the vertical axis. Exhibit 17A-4 illustrates this projection by the horizontal arrows for the observation at the event day and day −12, which we use as an illustrative, nonevent observation.

(c) Inference Based on Market-Model-Adjusted Abnormal Returns. Clearly, security returns react to causes other than the announcement under consideration. To assess the effect of the announcement itself, especially in Exhibit 17A-4, the study should ideally identify those causes and remove their effects from the data. In most cases, however, analysts cannot identify all relevant factors, let alone measure their cumulative effect on each observed return. Instead, analysts could use a more practical approach of comparing the return series under consideration to a benchmark series that the same common causes, other than the announcement, will likely affect. The return to a market index offers such a convenient, theoretically defensible benchmark return series. Any factor that affects all securities in the market, including those of the subject firm, will be reflected in the market index return series as well as in the return series under consideration.

Exhibit 17A-5 shows a scatterplot of Firm B returns against those to a market index. Each observation reflects the intersection of the firm return (shown on the

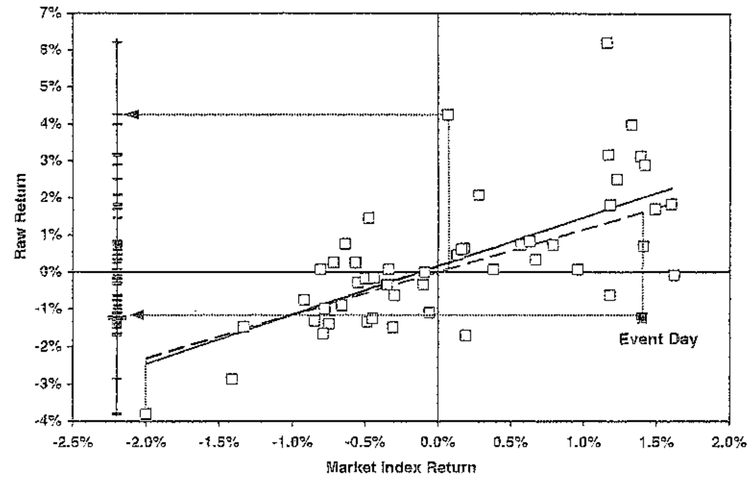

Exhibit 17A-5. Firm B Returns versus Market Index Returns

vertical axis) with the market return on the same day (shown on the horizontal axis). Exhibits 17A-4 and 17A-5 reduce to identical dot diagrams, but they have different horizontal axes. In Exhibit 17A-4, the horizontal axis carried information about which day (relative to the event day) related to a given return; this information bears no relevance for inferring the materiality of the event return. In Exhibit 17A-5, the horizontal axis carries information about market returns; the upward sloping configuration of the X–Y graph indicates that the market returns help explain variation in the firm's returns attributable to common, marketwide factors.

The dashed, sloping line in Exhibit 17A-5 shows the underlying, true regression of Firm B's returns on the market return—that is, the straight line that best explains the relation of the two return series. This line also provides a *forecast* of the firm's return, given only the market return and the joint history of returns. This forecast reflects the common factors that the market return indicates. (The solid sloping line in this exhibit reflects the approximate regression line that an analyst would calculate by applying a statistical estimation method to the available data shown in the exhibit. We discuss the distinction between the two lines in section 17A.3.)

An actual event-day return includes the effect of the announcement as well as the effects of the common factors. Therefore, the *difference* between the conditional forecast (i.e., the sloping line) and the actual return more nearly isolates the effect of the announcement alone than does the original raw return. The dotted vertical line connecting the event-day return to the regression line indicates this "residual" difference, which analysts often refer to as the *abnormal return* associated with the

announcement. The most negative nonevent market-adjusted return (in the lower left-hand corner of Exhibit 17A-5, and corresponding to event date +4 in Exhibit 17A-4) clearly becomes much less prominent than its raw return counterpart in Exhibit 17A-4. Exhibit 17A-5 largely attributes the unusual magnitude of this nonevent raw return to the effect of marketwide rather than firm-specific factors. Finally, Exhibit 17A-6 shows the time series and the corresponding dot diagram of abnormal (or "residual") returns to Firm B, confirming an unusual event-day return, once the analysis accounts for market-wide influences. Indeed, the market adjustment shown in Exhibit 17A-5 reveals the event-day abnormal return shown in Exhibit 17A-6 as the most extreme negative value observed during the sample period, unlike the corresponding raw return in Exhibit 17A-5. In contrast, Exhibits 17A-5 and 17A-6 show that the illustrative nonevent return on day −12 is little changed by market adjustment. (The day −12 return—like that on day 20—may or may not be associated with an identifiable disclosure of good news concerning Firm B but is, in any case, not associated with the day 0 event that we assume to be the analyst's focus.)

17A.2 APPLYING EVENT STUDIES TO LITIGATION QUESTIONS.

Event studies may serve at least two related purposes in disputes or litigation that involve damages calculations. First, the analysis can address whether, as a threshold question, a statistically reliable link exists between the information event in question and stock price responses. For example, in securities litigation over allegedly defective disclosures, financial experts must link the share-price reaction to the alleged correction of the allegedly defective prior disclosures or omissions. Opposing experts will break this link if they can attribute the price change at the alleged correction

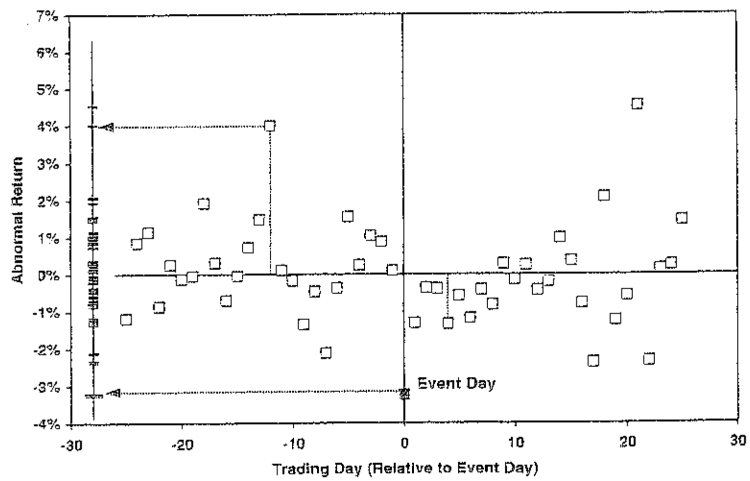

Exhibit 17A-6. Adnormal Returns to Firm B: Announcement at Day 0

date to other information revealed at the same time. Second, the analysis can address the magnitude of damages. An event study in this context proves most useful under the following three conditions:

1. A returns series is available that covers the event at issue and it is long enough to allow estimation of the market model (possibly augmented by an industry index, a size index, or a book-to-market index), including the firm-specific variability of returns (i.e., the standard deviation of returns after abstracting from market and industry effects).
2. The stock trades often enough so that each return covers just one day or at most a few days.
3. The parties can unambiguously identify the event in question with one or more announcements that have relatively certain timing, and the event announcement(s) do not contain a great deal of valuation-relevant information unrelated to the issue in question. Such unrelated information is commonly called *confounding events*. For example, a firm might announce a change in dividend policy concurrently with an earnings release; a study that focuses on the earnings release will have difficulty separating the earnings effect from that of the shift in dividend policy.

(a) Conditions for Using Event Studies in Litigation

(i) Statistical Conditions. As a practical matter, analysts will have more difficulty identifying interventions in return series for a thinly traded security because fewer returns exist and because (for multiday returns) all the news events during the several days covered by each return can affect that return. In addition, the variability inherent in a return series becomes a function of the time period covered by the return: that is, two-day returns have twice the variance of one-day returns and so on. Thus, analysts can most easily perform event studies for exchange-listed and NASDAQ common stocks that trade actively. Such studies become problematic for securities, such as debt and preferred stock, which tend to trade infrequently and at irregular intervals. In addition, analysts will find returns data much more readily available for common stocks than for many preferred stocks and most bonds.

(ii) Theoretical Conditions. Three theoretical conditions underlie the event study approach. First, the security of interest must be traded in an *informationally efficient market*: that is, one in which prices respond quickly and appropriately to valuation-relevant news. In such a market, current price provides the best estimate of intrinsic value for the security, conditional on publicly available information. Without this condition, it becomes difficult to argue that new information drives price changes. One party might propose, for example, that prices change not in response to underlying economic events but rather in response to fads or psychological factors that have little or no relation to economic events.

The efficient market condition provides the foundation of the fraud-on-the-market theory described in, for example, Koslow (1990). Plaintiffs in securities actions can rely on prices as measures of the intrinsic values of the underlying

securities, provided those prices appropriately impound public information. A person or entity that omits or falsifies information violates the integrity of prices; that is, the prices no longer represent intrinsic value. Under the fraud-on-the-market theory, an investor who relied on a price that did not reflect all relevant valuation information because of defective (i.e., false, misleading, or incomplete) disclosures can bring a claim, regardless of whether that person actually read or knew about the filings or other statements that contained the omission or false information. Note that if a defendant were to demonstrate that the security in question was not traded in an efficient market, the plaintiff's analysis would not meet an important condition for relying on event studies.

The second condition is that the analysts' statistical model of the return-generating process must be descriptively valid throughout the sample period. Event studies often use a form of market model as the assumed return-generating process, as illustrated in Exhibit 17A-5. Sections 17A.3 and 17A.4 of this chapter discuss the market model and issues related to the design of studies based on it.

The third condition applies most strongly where the plaintiff alleges that damages stem from a delayed disclosure. To base inferences on an event study of the actual disclosure, the abnormal return at the actual disclosure must be the same or nearly the same as the response that would have occurred had the disclosure happened at some other time. This condition requires stationarity in the reaction of the return series to disclosures, just as the second condition requires stationarity in the relation between the returns of the stock being analyzed and the market and industry index returns.

17A.3 STATISTICAL SPECIFICATION AND ESTIMATION OF EVENT STUDIES.

As illustrated in Exhibit 17A-5, an event study of security returns typically seeks to partition the variation of returns to a given security into two components. This partitioning corresponds to the *market model* equation for security s:

$$r_{st} = a_s + \beta_s r_{mt} + \varepsilon_{st} \tag{1}$$

where

- r_{st} = return to security s on day t
- a_s = market model intercept for security s, that is, the intercept of the dashed sloping line in Exhibit 17A-5
- β_s = market model beta for security s, that is, the slope coefficient of the dashed line in Exhibit 17A-5
- r_{mt} = market index return on day t
- ε_{st} = firm-specific abnormal return to security s on day t, that is, the vertical deviation of the observation for day t from the dashed sloping line in Exhibit 17A-5

The variation over time of $(a_s + \beta_s r_{mt})$, the first component of r_{st}, reflects marketwide effects. Analysts refer to this component as the systematic (or predictable, conditional on knowledge of the market or industry effects) portion of the return. The second component, ε_{st}, is the firm-specific effect, including that for $t = 0$ (the event day), the effect of the intervention in the return series. This idiosyncratic return component corresponds to the dotted vertical line in Exhibit 17A-5; it numerically measures the abnormal return. This terminology has intuitive appeal because the firm-specific return at a given event indeed lies outside the normal expected return predicted by or associated with industry and market factors.

(a) Estimating the Market Model by Statistical Regression Analysis. Analysts compute systematic or predicted returns based on the historical relation between a given stock's return and the returns on one or more market, size, book-to-market and industry indexes.[2] The estimated coefficients obtained by regressing a series of stock returns on returns to the selected index(es) summarize this historical relation. The dashed regression line in Exhibit 17A-5 represents the underlying true regression line; that is, we assume that it is a stable attribute of the process that generated the underlying series of prices but that its intercept and slope are unknown and must be estimated using the available data series. We estimate the intercept and the slope of the regression line by applying a statistical estimation method such as ordinary least-squares (OLS) to a sample of returns taken from an estimation period.

The solid sloping line in Exhibit 17A-5 shows the OLS estimate of the regression line, based on all data in the exhibit except the event-day return. Thus the solid sloped line corresponds to the *estimated* market model equation.

$$r_{st} = a_s + b_s r_{mt} + e_{st} \tag{2}$$

where a_s and b_s are the OLS estimates of a_s and β_s in (1). Given a_s, b_s, r_{st}, and r_{mt}, we can compute e_{st}, the estimated counterpart of ε_{st}. Whether e_{st}, is a *residual* or a *prediction error* depends on whether the estimation sample includes the return for day t. Because the analyst usually needs to estimate the market model as the normal benchmark for assessing the intervention in the return series, the study most often computes the estimated event-day abnormal return e_{s0} as a prediction error. That is, we compute a_s and b_s from an estimation sample without the event-day return, and compute e_{s0} as the deviation of r_{s0} from the estimated regression line shown as the solid line in Exhibit 17A-5:

$$e_{s0} = r_{s0} - (a_s + b_s r_{m0}) \tag{3}$$

(Note that the dotted vertical line at the event day in Exhibit 17A-5 shows the deviation of r_{s0}, the event-day return, from the *true* regression line, whereas e_{s0} is actually the deviation of r_{s0} from the *estimated regression* line. Both deviations are referred to as "abnormal returns," depending on the context.)

Analyses often specify the market model with a single, marketwide index (such as the Standard and Poor's [S&P] 500 or the value-weighted or equally weighted return on all New York Stock Exchange [NYSE] stocks). Studies can augment this basic model by including an industry index—for example, one composed of firms in the same Standard Industrial Classification (SIC) code which presumably react to the same industry-specific common factors. The resulting *augmented market model*

$$r_{st} = a_s + \beta_{ms} r_{mt} + \beta_{is} r_{it} + \varepsilon_{st} \tag{4}$$

has two slope coefficients, one for the relation of the stock return being analyzed with the market index and one for its relation with the industry index r_{it}. Both coefficients measure the sensitivity of stock returns to index returns. Fama and French (1992, 1993) have proposed a *three-factor market model*, which augments the marketwide index with indexes based on size (i.e., market capitalization) and book-to-market ratios. Studies include these additional factors (industry indexes, size-based indexes, and book-to-market indexes) to increase the proportion of the variance of total returns (the left-hand side variable) that they explain, thereby reducing the variance of the abnormal returns. Whether it is necessary or appropriate to include such additional factors depends on the context and is generally an empirical issue.

(b) Interpreting the Strength of the Market-Model Relation. Analysts use two measures of the estimated relation between individual security returns and returns to the index (or indexes): the magnitude (and statistical significance) of the regression slope (beta) coefficient(s), and the explanatory power of the regression expressed in terms of R^2, the fraction of the variance of returns explained by the regression.

(i) Beta Coefficient. In the context of the market model, analysts refer to the slope coefficient (β_s in Equation 1) as the security's *beta* or systematic risk. A beta coefficient of 1.0 means that the security returns change, on average, exactly as much as the market index returns in response to the common factors captured in the index. Coefficients greater or less than 1.0 indicate securities whose expected returns vary more or less than average in response to the common factors. Exhibit 17A-7 summarizes the distribution of estimated betas for NYSE/AMEX and NASDAQ securities in the period 1985 to 1999, organized by size decile. Size Decile 1 in Exhibit 17A-7 refers to those 10 percent of exchange-listed firms having the smallest market capitalizations on the indicated exchange; similarly, decile 10 refers to those 10 percent of listed firms having the greatest capitalizations. The second of the three groups of columns of the exhibit summarizes, separately for each firm size group, the range of beta values for that group. Specifically, the exhibit shows the first quartile, median, and third quartile (i.e., the 25 percent, 50 percent and 75 percent points) of the range of calculated beta values. For example, the exhibit shows that the median calculated beta among all NYSE/AMEX firms was 0.67.

In principle, the weighted average measure of sensitivity of stock returns to market returns (i.e., the average beta) equals 1.0; few stocks have zero or negative estimated sensitivity measures, and few stocks (less than one-fourth) have sensitivity measures substantially greater than 1.0.

(ii) Statistical Significance. The statistical significance of the estimated coefficient measures the reliability of its estimated sign and magnitude, based on the coefficient's standard error. The OLS regression calculations produce a standard error for each estimated coefficient to gauge the size of the discrepancy between the estimated value and the underlying true value. (This discrepancy is the difference between the slopes of the solid OLS regression line in Exhibit 17A-5 and the dashed true regression line.)

Listing	Size Decile	Standard Deviation of Raw Returns (%)			Market Model Beta			Market Model R-Squared (%)		
		Q1	Median	Q3	Q1	Median	Q3	Q1	Median	Q3
NYSE/AMEX	1	3.0	4.8	6.7	0.24	0.49	0.78	0.2	0.5	1.4
	2	2.3	3.6	5.2	0.26	0.53	0.83	0.4	1.1	2.6
	All	1.7	2.3	3.4	0.37	0.67	1.00	1.1	3.5	9.0
	9	1.6	2.0	2.4	0.64	0.91	1.15	7.2	12.2	18.1
	10	1.6	1.8	2.1	0.81	1.01	1.20	15.2	23.0	28.5
NASDAQ	1	4.3	5.9	7.8	0.00	0.25	0.57	0.0	0.1	0.5
	2	5.0	6.4	8.5	0.12	0.37	0.71	0.0	0.2	0.6
	All	3.3	4.6	6.3	0.25	0.57	0.97	0.2	0.8	2.7
	9	2.5	3.4	4.6	0.55	0.91	1.28	1.6	3.4	5.9
	10	2.2	3.2	4.1	0.78	1.19	1.67	4.1	7.1	11.2

Notes: Data are taken from the files of the Center for Research in Security Prices (CRSP) at the University of Chicago, including Scholes-Williams estimates of market model betas based on the value-weighted CRSP index. Size is measured by market capitalization; the smallest firms are in decile 1. Q1 and Q3 denote the first and third quartiles of the data.

Exhibit 17A-7. Summary Statistics for Estimates of Beta, Standard Deviation of Raw Returns, and Market Model R^2 by Capitalization Decile: NYSE & AMEX NASDAQ Firms, 1985–1999

Statistical theory provides that a confidence interval centered at b_s and extending for two standard errors in each direction will cover the true value β_s approximately 95 percent of the time. Thus a finding that the two-standard-errors interval does not cover zero indicates at a 95 percent level of confidence that the estimated coefficient b_s has at least the same sign as the true coefficient β_s. In many applications, one can restate this condition as that of requiring that the *t ratio*, that is, the ratio of the estimated coefficient to its standard error, equal at least two.

Most event studies have greater interest in the statistical significance of the estimated effect of the intervention (i.e., the significance of the abnormal return) than that of the slope coefficient, as discussed in section 17A.3(c), which explains event parameters.

(iii) Standard Deviation of the Return Series. The presence of return variability that differs substantially across stocks means that a return of a given magnitude may be unusual for one firm and frequently observed for another. A common measure of the variability inherent in a given return series is the *time-series standard deviation* of returns, which measures the typical dispersion of returns around their mean over some time period.[3] Exhibit 17A-7 summarizes the standard deviations of daily returns for NYSE/AMEX and NASDAQ firms, overall and for extreme market-value deciles. These standard deviations decrease as firm size increases (larger firms have smaller variability of returns). The largest standard deviations occur for smaller firms on both the NYSE/AMEX and NASDAQ (with a median of about 4 to 5 percent for the smallest NYSE/AMEX firms and a median of about 6 percent for the smallest NASDAQ firms), while the smallest standard deviations occur for the largest firms on the NYSE (with a median of about 1.8 percent).

Statistical theory shows that approximately two-thirds of all returns should fall within plus-or-minus one standard deviation of the mean, and about 95 percent of all returns should fall within plus-or-minus two standard deviations. Thus, one would observe by chance (i.e., on a day selected at random) a return that exceeded two standard deviations from the mean no more than 5 percent of the time.

We can use this approximate rule and the data in Exhibits 17A-3 and 17A-4 to evaluate the likelihood of observing the event-day returns to Firms A and B by chance alone; that is, on the assumption that the information events had *no* effect on the return series. The sample standard deviations of nonevent returns for Firms A and B in the examples equal 2.13 and 1.84 percent; their sample average returns equal .16 and .35 percent. Firm A's event-day return of 12.1 percent deviates by more than five standard deviations from the average. Thus, we have over 95 percent (indeed, more than 99.9 percent) confidence that the event-day return is not simply a random observation from the distribution of nonevent returns, unaffected by the information event. Firm B's event-day return of −1.3 percent, however, lies less than one standard deviation from the average; one frequently observes returns as extreme as −1.3 percent even when no news announcement occurs.

(iv) Explanatory Power: R^2. The second numerical measure of the estimated relation between returns of a stock under study and the returns to an index or indexes lies in the explanatory power of the regression. The regression's R^2 (R-squared) measures (in the case of a market model regression) the fraction of the variance in

stock returns that the index returns explain. The explanatory power appears as a reduction in the variance of abnormal returns [for example, the e_{st} in equation (2)] relative to the variance in raw returns. For example, the time-series variance of Firm B's raw returns plotted in Exhibit 17A-4 is approximately .000340 (excluding the event-day return), while that of the abnormal returns in Exhibit 17A-6 is .000179. Therefore the R^2 of the regression in Exhibit 17A-5 equals approximately 47 percent (i.e., 1 − .000179/.000340). The greater the R^2, the greater the portion of a given stock's return that one would expect to arise from market and industry forces, as opposed to firm-specific news.

In a market model regression, the portion of a stock's return not explained by the chosen indexes is called *residual variation*. Both the amount of total variation in stock returns and the amount of residual variation in stock returns differ among stocks. Exhibit 17A-7 summarizes the market model R^2 for individual firms on the NYSE/AMEX and NASDAQ. For the largest NYSE firms the median R^2 equals about 23 percent. The overall median R^2 for NYSE/AMEX firms equals only about 3.5 percent, however, and for NASDAQ firms it equals only about .80 percent. Because of averaging effects, stock portfolios have much greater R^2 numbers than those for individual stocks.

(c) Inferring the Effect of the Event: The Event Parameter Approach. The event-day abnormal return of −3.0 percent shown in Exhibit 17A-6 deviates from the mean nonevent abnormal return (.24 percent) by about 2.4 times the standard deviation of the nonevent abnormal returns (1.34 percent). By this measure, the event-day abnormal return is statistically significant at the 5 percent level.

Although similar in form to the assessment of statistical significance for the raw event-day returns as outlined above, the calculation for abnormal returns as presented here is not feasible. In a real event study, analysts will not know the underlying, "true" regression represented by the dashed line in Exhibit 17A-5 and, therefore, will not know the length of the vertical line segment representing the "true" event-day abnormal return. Although analysts will know none of the ingredients of the sample calculation outlined here, they can approximate all of them by using the estimated, solid regression line in Exhibit 17A-5.

Using the *estimated* abnormal returns complicates the problem of statistical inference because the *estimated* event-day abnormal return has two components: (1) the true abnormal return, which includes the economic effect of the information event and an error component attributable to Firm B's inherent, residual return variation; (2) error attributable to the deviation between the known, estimated regression line and the unknown, true line shown in Exhibit 17A-5. Thus a straightforwardly computed t ratio for the event-day abnormal return cannot have the same interpretation as the t ratio for a raw return.

An exact formula exists for adjusting the t ratio for the abnormal return so as to give it the same interpretation as that for the raw return [see Patell (1976)]. The event parameters method provides an equivalent, alternative approach that may prove more computationally convenient for this and other purposes. Specifically, consider the augmented market model

$$r_{st} = a_s + \beta_s r_{mt} + \gamma_{sA} d_{At} \varepsilon_{st} \qquad (5)$$

where d_{At} is an indicator variable for the date of the information event under study; that is, d_{At} equals one on the date of announcement a and zero on all other dates. Analysts often refer to the coefficient γ_{sA} as an *event parameter*, because it measures the sensitivity of returns of security s to information events of type A.

Let g_{sA} be the OLS estimate of γ_{sA} in a regression that includes both the original estimation sample for Equation 2 and the event-date observation. This event parameter g_{sA} equals the prediction error at the event date as defined previously, and its standard error and t ratio account for the effect of the estimation error in the regression line, thus applying the Patell (1976) adjustment automatically.

(d) The Statistical Power of the Event Study Method. The preceding discussion illustrates how one can estimate the market model and use it to adjust a raw return series for market and industry effects that obscure the firm-specific effect of a given announcement. Even if the investigator knew the precise market model coefficients, the residual variation of the return series would still obscure the announcement effect. Exhibit 17A-7 shows, moreover, that the market model has modest explanatory power. (The summarized R-squared data show that among the largest NYSE/AMEX firms the model explains 28.5 percent of the variance of the security returns or more for only one quarter of the firms in that size group.) Thus, one would naturally consider to what extent the market-model adjustment sharpens inferences regarding the presence of an announcement effect.

We can examine this issue by comparing the statistical power of tests for an announcement effect, with and without market adjustment. For a given true economic effect (for example, –2.05 percent, the true value of the "event-related" perturbation in the illustrative, simulated data underlying Exhibit 17A-4), the power of a statistical test is the probability of concluding that an effect did indeed occur. Thus, the power of the rule requiring a t ratio greater than 2 is the probability that a t ratio of this magnitude will actually occur, given the presence of a true effect of –2.05 percent.

Exhibit 17A-8 shows the theoretical values of such power probabilities under several assumptions regarding the R^2 of the market model, the magnitude of the true effect, and the value of the market index return r_{m0} on the event day relative to the mean market return. For example, if the true effect equals zero, then the probability of a t ratio greater than 2 is .046 in all cases. This simply reflects the 95 percent correctness of the approximate statistical rule: The probability of concluding that an effect is not present when, in fact, there is none is 1 – .046 ≈ 95 percent. For a true effect equal to twice the standard deviation of raw returns, r_{st}, the probability of detection ranges from .492 to .788, increasing as the explanatory power of the regression (R^2) increases. Comparing the empirical distributions of R^2 in Exhibits 17A-7 and 17A-8 suggests that for many firms, and in particular for most small firms, the market-model adjustment has little effect on the power of the test. (The benchmark for this comparison occurs when $R^2 = 0$; that is, no market adjustment.) Whether this will hold true in a given litigation setting is, of course, an empirical question.

(e) Confidence Interval for the Price Effect. Given the data shown in Exhibit 17A-5, the estimated event parameter g_{sA} equals –3.49 percent, with a corresponding t ratio of –2.53. (The difference between the true abnormal return of –3.0 percent

		"True Effect" (× $\sigma(r_{st})$)			
	R^2	0	1	2	3
$\Delta r_{mt} = 0 \times \sigma(r_{mt})$:					
	0%	0.046	0.158	0.492	0.834
	1%	0.046	0.159	0.496	0.838
	2%	0.046	0.160	0.500	0.841
	5%	0.046	0.164	0.513	0.853
	10%	0.046	0.171	0.535	0.871
	20%	0.046	0.187	0.585	0.907
	50%	0.046	0.275	0.788	0.986
$\Delta r_{mt} = 2 \times \sigma(r_{mt})$:					
	0%	0.046	0.149	0.463	0.805
	1%	0.046	0.150	0.467	0.809
	2%	0.046	0.151	0.471	0.813
	5%	0.046	0.155	0.483	0.825
	10%	0.046	0.161	0.504	0.845
	20%	0.046	0.176	0.553	0.885
	50%	0.046	0.258	0.757	0.980

Exhibit 17A-8. Effects of Market Adjustment of Raw Returns on the Power of Tests for Event Responses in Security Returns

and the estimate of –3.49 percent results from the discrepancy between the true and the estimated regression lines at the event date.) The t ratio indicates that the difference of –3.49 percent between the return predicted for the event day by the estimated regression line (2.21 percent) and the event day return that actually occurred (–1.28 percent) is statistically significant at the 5 percent level. Thus the discrepancy will not likely have occurred as a random observation from the distribution of deviations from the regression line that occurred during the non-event period. This result offers evidence of the economic effect of the information event. We can use statistical measures to describe the precision of this evidence. In this example, the 50 percent confidence interval for γ_{sA} ranges from –4.43 percent to –2.56 percent and the 95 percent confidence interval from –6.26 percent to –.72 percent.

Suppose, for example, that the closing price of security s on the day before the information event was \$100. Since the total event-day return was –1.28 percent, security s closed at \$98.72 on the event day. Given the market return on the event day, the market-model regression predicts a return of 2.21 percent and, accordingly, a closing price of \$102.21. The statistical significance of the event parameter shows that we can conclude with 95 percent confidence that the value of security s declined as a result of the information event. Our point estimate of \$3.49 offers the

best linear unbiased estimate of the price effect of the announcement.[4] We can also use statistical measures to describe the precision of this estimate. Specifically, given the estimate and its standard error, we can infer with 50 percent confidence that the true price effect (a decline) lies in the interval from $2.56 to $4.43 and with 95 percent confidence that the true effect lies in the interval from $0.72 to $6.26 per share. The wide range of prices in the 95 percent confidence interval reflects the inherent high variability of underlying price movements.

(f) Extensions to More Complicated Settings. One could also extend the event parameter approach to more complicated situations than that illustrated here. For example, an analyst can use the approach to examine the *cumulative abnormal return* (CAR) over multiple event days, as might be the case if the date of a disclosure were uncertain or if the study needed to consider several disclosures [see Marais, Schipper, and Smith (1989)]. In addition, analysts can estimate event parameters for portfolios of firms, weighting such portfolios to highlight various economic features of the securities involved. Examples of such weighting schemes include weighting by income effects of a given accounting disclosure or weighting by tax benefits to be received from an announced tax change. Schipper, Thompson, and Weil (1987) provide both a formula for computing economically weighted portfolios and an application that includes multiple event days. Finally, the regression structure of the event parameter approach can be exploited. Specifically, since the approach focuses on regression coefficients rather than residuals or prediction errors, one can apply a number of standard econometric methods derived for the regression framework.

We have found the event parameter approach relatively more convenient than the residual analysis approach for dealing with securities that are infrequently and nonsynchronously traded, such as bonds and preferred stock. These securities present their own special estimation problems (for an example, see Marais, Schipper, and Smith, 1989).

17A.4 DESIGN ISSUES IN EVENT STUDIES. The event study originated in academic research, so the conventional design features and assumptions of event studies fit academic research questions. They may not, however, fit litigation-based questions. The legal setting may present a problem or issue that seems familiar in the sense that it resembles a conventional research issue. But the special features that a litigation assignment presents may mean that the problem will not submit to conventional formulation. In such cases, the analyst must develop approaches that fit the legal setting. This section describes a number of design issues, some of which occur in both academic research and litigation applications of event studies, and some of which one will generally find only in litigation settings.

(a) Inferences about Magnitude and Statistical Significance. Event studies may serve both to establish whether any effect occurred and to quantify the effect, if any, including the margin of error of the measured effect. In the former purpose, the question of *statistical significance* of firm-specific returns (abnormal returns or prediction errors) becomes fundamental. For the latter purpose, the *magnitude* of such returns becomes the basic input to the measurement calculation, and one can use statistical significance as an indicator of the confidence interval around the point estimate of the effect.

This chapter has already discussed the details of statistical inference. When considering design issues, it suffices to note that inference involves drawing conclusions about the likelihood that the analysis would yield the given result by chance, given assumptions about the data. Thus, if the abnormal return computed at a spin-off announcement is +2.6 percent, one question of inference involves the frequency with which one observes returns this large or larger in the absence of spin-off announcements. Analysts can answer this question by assessing the statistical significance of the abnormal return; one can do this by computing and interpreting a t ratio of the event period abnormal return divided by the time-series standard deviation of non-event period abnormal returns. A related question of inference involves quantifying our uncertainty about the number itself; that is, given the inherent error in any statistical estimate, how likely is it that the true abnormal return was, say, +1.5 percent or +3.2 percent?

It follows, then, that inferences about significance and magnitude are linked to the existence of economic effects. Suppose, for example, the event study produces an announcement-related abnormal return estimate of −.50 percent and the standard deviation of such abnormal returns equals .39. This yields a t ratio of about −1.28, so in larger samples of returns, one would observe abnormal returns of this size or larger in absolute value about 20 percent of the time. In addition, we have approximately 95 percent confidence that the true return lies between −.28 percent and +1.28 percent (that is, plus or minus two standard deviations from the estimated abnormal return). Given these results, one would find it difficult to argue for any discernible intervention in the return series at the event announcement. The most likely firm-specific effect of the event equals approximately zero.

(b) Residual Return for a Single Firm. When one performs an event study in the context of a dispute or litigation, the analysis frequently includes only one firm. In such an analysis, one cannot compensate for the idiosyncratic characteristics of the subject firm's return series as one would in academic studies that use large samples spread over time and industries. The variability in the return series, usually measured by the time-series standard deviation of returns, differs across firms and sometimes over time for a given firm. Trading patterns also differ across time and firms; even on the NYSE some listed firms trade relatively infrequently and, hence, one cannot obtain reliable daily prices.

The technical difficulties arising from the variability of returns are reduced to the extent that the study can attribute a portion of this variability to market or industry factors. The quantitative measure of this explanatory power is R^2, discussed in section 17A.3(b)(*iv*). To the extent the analysis includes market factors, industry factors, or other factors that explain the variance in the stock or security returns being analyzed, *residual* or *idiosyncratic return* variance is smaller than the raw return variance. The measure of residual return is the error term in the (possibly augmented) market model regression. Residual return variability (the portion

of return idiosyncratic to that stock or security) increases the width of the confidence interval around any given abnormal return estimate. The greater the residual return variability, the greater the magnitude of any prediction error or abnormal return required to support the argument that a given information event affected the stock return.

(c) Choice of Event Period. The number of days around any information announcement for which the study will compute abnormal returns defines the length of the event period or event window.[5] One needs to choose an event window that is *both* known for certain to contain the information release of interest and believed to contain no other valuation-relevant information. The first objective implies a longer event window and the second implies a shorter one. The choice may involve trading off more variability in the series from choosing a longer window, against dealing with some uncertainty about precisely when the news in question reached market agents.

In an ideal case, one can trace the announcement to a time-stamped press release, so the study can use a single day as the event window. In other cases, however, a daily newspaper such as the *Wall Street Journal* becomes the source of event information. On any given day, the newspaper carries stories from the previous day; news in those stories will already have been reflected in share prices if the stories emerged during trading hours. News in stories released after the close of trade will affect share prices the day they appear in the newspaper. For this reason, studies conventionally use two-day event windows when a newspaper becomes the source of news announcements. Not all news events appear in newspapers, however. Such potentially important information sources whose contents rarely appear in newspapers include proxy statements containing information about compensation plan changes or antitakeover amendments; Form 10Ks containing detailed income statement information; analyst reports assessing the firm's future probability. Event studies based on such disclosures have used longer event windows.

(d) Choice of Estimation Period. Event studies usually involve the calculation of sensitivity measures (beta coefficients) using a market model, or possibly a market model augmented by some combination of an industry, size and/or book-to-market ratio indexes. Exceptions may occur in special circumstances, however. For example, analysts may occasionally conduct event studies when the available returns data are too limited to permit elaborate statistical estimation; in some such cases researchers and analysts have used the marketwide average sensitivity coefficient of 1.0, in which case they simply subtract the index return from the firm-specific raw return. Also, market model estimation and analysis may reveal that marketwide factors (possibly augmented) explain virtually none of the variance of returns of the series of interest. In such a case all, or nearly all, of the firm's return is idiosyncratic and the effect of extracting marketwide factors using an augmented market model may prove negligible.

If the study will estimate a market model, the length of estimation period becomes a design choice. A long estimation period may yield unstable results,

because the series includes a number of interventions unrelated to the litigation question at hand. Such interventions include major restructurings, write-offs, and acquisitions. A study that uses an excessively short series may lead to an imprecise estimation.

A litigation setting increases the difficulties of estimation period choice if the behavior of returns after the event being analyzed differs materially from that of returns before it, as would be the case, for example, if the firm filed for protection under the bankruptcy laws shortly after the event. In this case, the study cannot include returns after the event in the estimation period. Analysts also cannot include returns during a period of alleged fraud.

Clearly, analysts must consider the specific facts and circumstances of the given litigation question in choosing the estimation period. No specific rule or procedure will likely suit every situation. Based on academic research, however, it appears that estimation periods containing fewer than 50 returns will likely yield imprecise estimates.

(e) Choice of Indexes. A final design issue is the choice of indexes. Using commercially available data sources, one can match exchange-listed firms with an index of like firms and match NASDAQ firms with an index of all NASDAQ firms. The wealth of research demonstrating the effects of firm size on share returns indicates that it may be useful to take account of size explicitly in computing abnormal returns. One can do this either by including as an index a portfolio of firms of about the same size as the firm being analyzed, or by choosing appropriately between an equally weighted market index and a value-weighted market index. The former gives equal weight to all firms, so larger firms do not dominate the index value. The latter strongly reflects the return performance of the very largest firms.

If the study will include an industry index and commercial vendors of data do not provide a suitably specific industry index, a litigation setting may require that the analyst develop a specialized index and prepare extensive explanatory materials supporting the choices made in the development. The SIC code, which provides broad industry membership based on a dominant industry, offers a reasonable starting point. One could obtain a finer partitioning obtained from industry analyses provided by, for example, *Value Line* or *Standard and Poors*. The firm under study may also self-report a peer group in its proxy statement disclosures of performance-based compensation. In any case, the analyst must develop a peer group whose returns one would expect to move with the same industry forces as do the returns of the firm being analyzed. One can then more reasonably attribute the deviations of the subject firm's returns from the industry's returns to the firm-specific news.

Finally, Fama and French (1992, 1993) report that market model betas have relatively modest explanatory power for both cross-sections and time-series of stock returns and time-series of bond returns, relative to the explanatory power of size and the book-to-market ratio (the ratio of the book value of common equity to its market value) for stocks and term-structure factors for bonds. These results suggest that, in litigation settings, one may want to include controls for size and book-to-market ratios for stocks and for term-structure factors for bonds.

17A.5 LINKING THE EVENT STUDY TO PEER-REVIEWED, PUBLISHED RESEARCH. Applications of event studies abound in the finance and accounting research literature, which has long used them as a tool for measuring the shareholder wealth effects of announcements by and about firms, or of government actions affecting firms [see MacKinlay (1997)]. For example, Pincus (1997) performs an event study analysis of the Revenue Acts of 1938 and 1939 to assess the shareholder wealth effects of legislation that permitted last-in, first-out (LIFO) inventory valuations for tax purposes. Other examples of the wide variety of corporate events that the literature has studied include tender offers, spin-off announcements, leveraged buyout announcements, poison pill adoptions, golden parachute adoptions, and announcements of defensive tactics in takeover contests. Some of these published studies may pertain to the issues in a given dispute or litigation. Journals have also published numerous summaries and reviews of such potentially relevant literature: see, for example, Weston, Chung, and Siu (1998) and Binder (1998).

Some articles have extended event study methods to analyze price effects on securities other than common stock [see, for example, Marais, Schipper, and Smith (1989)]. Related methods have also been applied to nonprice effects of various kinds of interventions. For example, several studies have investigated the change in research and development spending after Generally Accepted Accounting Principles (GAAP) were changed in 1974 to require the immediate expensing instead of capitalization of such expenditures [Selto and Clouse (1985) review this research]. Another example is Scholes, Wilson, and Wolfson's (1990) analysis of changes in the municipal bond holdings of banks after changes in tax rules disallowed part of the interest deduction on debt incurred to buy such bonds.

(a) Benchmark Values. Suppose, for example, that the issues in a dispute or litigation involved the market-value effects of qualified audit opinions. This might occur if a plaintiff were suing a CPA firm for failing to issue an audit opinion with a going-concern qualification for a firm that subsequently filed for bankruptcy [see, for example, Carcello and Palmrose (1994)]. The academic literature contains event studies [for example, Dodd et al. (1984)] that document the average effect of qualified audit announcements in large samples of firms overall, that is, without conditioning on firm-specific circumstances. Such research provides an informative benchmark value for the expected effect of a given qualified audit announcement, but for the effects of firm-specific circumstances. An application to a specific litigation setting should, of course, account for any special, case-specific circumstances which might imply an effect different from the averages in large samples.

(b) Adapting Methods for Nonstandard Situations. One could also benefit from consulting the academic research literature because this literature contains peer-reviewed solutions to specific technical problems arising from a variety of special circumstances, some of which may parallel the issues in a given dispute or litigation context. The literature relevant for this purpose includes not only accounting and finance research using event studies but also the broader statistical literature on related methods; for example, see Box, Hunter, and Hunter (1978) on *intervention analysis* and Yao (1993) on the detection of *change points*. An illustration of special circumstances requiring nonstandard methods is provided by Marais, Schipper, and Smith (1989), who implement an event study method for analyzing multiple debt securities of a given firm. One could expect that a given event will similarly (if not identically) affect multiple bond issues of a single firm. Thus, the returns of different bonds of a single firm will likely exhibit cross-sectional correlation. Moreover, several other complications, including irregular and nonsynchronous trading, will hamper analyses of returns of these securities.

APPENDIX: EVENT STUDY TERMINOLOGY

This glossary defines various terms commonly used in the event study literature, including terms used interchangeably to describe the same (or nearly the same) concept.

Abnormal returns: See also *excess return, market-adjusted return, prediction error, residual return.* A firm-specific or idiosyncratic return, usually estimated at an event date or over an event period; the difference between the realized return during a period and the return expected given (1) returns to market (and possibly industry) indexes, and (2) the historical sensitivity of the firm's returns to the index returns.

Augmented market model: A market model with additional right-hand-side variables, such as industry indexes and indexes based on size or book-to-market ratios. See also *market model.*

Beta: Estimated slope coefficient from a time-series regression of common stock returns on market index returns; a measure of sensitivity of stock returns to market index returns. Sometimes called *systematic risk;* a parameter of the market model.

CAR: See *cumulative abnormal return.*

Confounding events: Information releases concurrent with, or very close in time to, a specific news announcement under investigation.

Cumulative abnormal return (CAR): Abnormal returns aggregated over some time interval. Over short periods, such as two or three days, analysts will often sum abnormal returns to compute cumulative abnormal returns. Over longer periods, analysts often use compounding.

Efficient market hypothesis: In its semistrong form, the view that stock prices quickly and unbiasedly move to reflect all relevant public information. Such a market makes arbitrage gains impossible. Alternatively, the view that prices measure intrinsic values unbiasedly (valuation errors, if they exist, are symmetric and centered on zero) and one cannot use public information to infer the existence and magnitude of any given stock's valuation error.

Estimation period, estimation window, estimation interval: Time period (measured in days, weeks, or months) over which returns are taken to estimate the parameters of the market model or augmented market model; can include returns before the event period, after the event period, or both.

Event parameter: An indicator variable included in a market model, taking on the value 1 for event periods and 0 (zero) otherwise. Captures the shift in returns on event days (weeks, months) as a measure of abnormal return.

Event period, event window: Time period (measured in days, weeks, or months) over which the study will compute abnormal returns. The study should choose the length of the period (window) to allow sufficient time for returns to impound information in the event announcement.
Event study: Empirical investigation and analysis of a time series of data, usually stock or security returns, for evidence of unusual behavior associated with identifiable news announcements.
Excess return: This term has two distinct uses. First, it may equal the difference between a firm's stock return and the return on a market index. In this usage, it becomes a synonym for *abnormal return* if the difference is computed as

[stock return] – [beta] × [market index return].

Second, it may equal the difference between a firm's stock return or an index return and a proxy for the risk-free rate of interest.
Market-adjusted return: Loosely, any difference between a stock or security return and the return on a market index.
Market index: An equally weighted or value-weighted portfolio intended to proxy for aggregate wealth. Examples include the S&P 500 and the Center for Research in Securities Prices (CRSP) indexes of exchange-listed stocks and over-the-counter (NASDAQ) stocks.
Market model: A time-series regression model where the left side variable is returns to a given stock or security (sometimes a portfolio of such stocks or securities) and the right side variable is returns to a market index. See also *augmented market model.*
Predicted return: Systematic return, or return to a stock or security expected given the stock's (security's) sensitivity to market (and possibly industry) index returns and the realized returns to those indexes.
Prediction error: See *abnormal return.*
Raw return: See *return.*
Residual return: Firm-specific return, idiosyncratic return. Sometimes used as a synonym for *abnormal return;* strictly speaking, however, an estimated residual from a market model regression.
Residual variation: The time-series variability of a return series that one cannot attribute to or explain by movements in a market index (sometimes a market index combined with an industry index).
Return: The holding period return for a given security in the period from $t - 1$ to t is

([Price at t] + [Distributions in period t] – [Price at $t - 1$]) ÷ [Price at $t - 1$]

Also referred to as *raw return,* to distinguish it from an abnormal return, or from the continuously compounded form

ln ([Price at t] + [Distributions in period t]) ÷ [Price at $t - 1$]).

Systematic return, predicted return: The return on a stock or security that one can attribute to movements in a market index, or sometimes a market index combined with an industry index.

Systematic risk: Beta (slope) coefficient from a market model regression; measure of return sensitivity to movements in a market index.
***t* ratio:** The ratio of an estimated coefficient or abnormal return to a measure of variability, such as the standard error of the estimated coefficient or the time-series standard deviation of abnormal returns.

NOTES

1. The total return to a share of common stock equals the percentage price change adjusted for cash dividends and for stock splits and stock dividends. Thus, if a cash dividend were paid on a given day, that day's return would include both the price change and the dividend. The percentage return would equal (price change + dividend)/(previous day's price). The treatment of stock splits and stock dividends becomes more complicated, in that these change the number of shares in an initial position. Commercial data services, such as the Center for Research in Security Prices at the University of Chicago Graduate School of Business, provide common stock returns series that fully account for cash dividends, stock splits and stock dividends. In addition, they provide documentation that would permit users to make their own adjustments to a price and dividend series.
2. Both the stock returns and the returns to the industry and market indexes can be raw, sometimes called *unadjusted,* or they can be returns in excess of a measure of the risk-free rate of interest (usually some return on a U.S. government treasury note). Analysts refer to returns adjusted by subtracting a risk-free rate as excess returns. Fama and French (1992, 1993) propose augmented market models which include size and book-to-market indexes.
3. The time-series *standard deviation* of the return series equals the square root of its time-series *variance.* Variance is a statistical measure of the spread or dispersion in a distribution. The mean or average captures the middle of the distribution, while the variance captures the tendency of observations to differ from the mean. A larger variance means a greater spread in the distribution. The variance of a given sample of observations is the average of the squared deviations of the sample values from the sample mean.
4. This is a consequence of the Gauss–Markov theorem; see, for example, Newbold (1984, Chapter 12).
5. This discussion assumes the use of daily returns, both for simplicity and because studies commonly use daily returns. One can, however, measure event periods in terms of other return intervals, such as weekly [see, for example, Patell (1976)] or monthly [see, for example, Schipper and Thompson (1983)].

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